LETTERS TO THE EDITOR.

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The Sacred Tree of Kum-Bum.

THE eighth number of the *Bulletin du Muséum d'Histoire Naturelle* for 1895, which has just reached me, contains a paper by M. Édouard Blanc, entitled "L'Arbre à prières de Goumboum." This reminds me of a brief article which I contributed to NATURE in 1883 (vol. xxvii. pp. 223, 224).

M. Blanc begins his account by the remark: "Je veux parler du fameux arbre qui croît dans un monastère buddhiste, au nord du Thibet, et qui produit des lettres, des mots, des prières et autres formules religieuses, le plus souvent tracées sur son écorce et sur ses feuilles." Of the actual fact he adds, "des voyageurs européens, M. Potanine et M. Grenard entre autres, ont apporté le témoignage de leur observation directe."

It is evident, then, that the tree still exists much as Huc and Gabet described it. And M. Blanc brought back with him to Europe a branch and a portion of the trunk. He says: "Le phénomène est véritable: il existe réellement, et j'ai vu des caractères thibétains très nettement tracés sur les branches de l'arbre en question."

M. Blanc discusses the cause. He dismisses the probability of their being either natural markings or the work of insects accidentally resembling alphabetic characters. He has no doubt that they are produced artificially, probably with the aid of heat.

In 1891 Mr. William Woodville Rockhill's book, "The Land of the Lamas," appeared; in it (pp. 67, 68) he gives the following account of the tree:—

"Although I did not see the convent treasure-house and the 'white sandal-wood tree' until later, I will describe them here. In a small yard enclosed within high walls stand three trees about twenty-five to thirty feet high, a low wall keeping the soil about their roots. These are the famous trees of Kum-Bum, or rather tree, for to the central one only is great reverence shown, as on its leaves appear outline images of Tsong-k'apa. The trees are probably, as conjectured by Kreitner,[1] lilacs (*Philadelphus coronarius*); the present ones are a second growth, the old stumps being still visible. There were unfortunately no leaves on the tree when I saw it; and on the bark, which in many places was curled up like birch or cherry bark, I could distinguish no impress of any sort, although Huc says that images (of Tibetan letters, not images of the god) were visible on it. The lamas sell the leaves, but those I bought were so much broken that nothing could be seen on them. I have it, however, from Mohammedans that on the green leaf these outline images are clearly discernible. It is noteworthy that whereas Huc found letters of the Tibetan alphabet on the leaves of this famous tree, there are now seen only images of Tsong-k'apa (or the Buddha ?). It would be interesting to learn the cause of this change."[2]

I was anxious to see what could be ascertained from the leaves brought back by Mr. Rockhill. An application to my friend Prof. Sargent, at Harvard, procured me the following interesting letter:

1914 *N Street, December* 23, 1893.

MY DEAR SARGENT,—As regards the famous Kum-Bum tree, I was not permitted, in any of my visits to it, to touch the tree, but I got a lot of leaves fallen from it, some of which I gave to the British Museum (Department of Ethnology), where Franks or Read would, I doubt not, be pleased to show them to Dyer.

From what the people at Kum-Bum told me, especially in view of their reference to the big bunches of violet flowers, I *thought* the tree might prove to be a lilac.

The bark turns up on the trunk like that of a birch. Kreitner is responsible for the identification of this "white sandal-wood" with the *Philadelphus coronarius*.

The roots from which the trees I saw were growing look very old, how old I cannot say, being ignorant in all such matters, the live stems are certainly not over 15 to 20 feet in height, and 4 to 6 inches diameter at the root, and some of them look very healthy. It may be that when Huc and Gabet visited this place (in 1842, I believe) the original trunk was yet alive.

They say that "three men could not stretch around the trunk," but he adds that it was not over 8 feet high. He must refer to an old dead trunk, out of which shoots were growing. If this is not the case, we cannot have seen the same tree; that is all there is about it.

As to the "odeur exquise et qui approche un peu de celle de la cannelle," this must be hearsay, and refers to the popular belief that the tree is a sandal-wood, or else is a native simile for the odour of lilacs.

The large red flowers Huc also refers to may be violet ones. Mongol is not so precise a language, in fact certain colours which which we would call violet are invariably called red by them.

Huc mentions the curling up of the bark.

On the whole, I am inclined to think that here as throughout his book, Huc's reminiscences of facts and hearsay have misled him. He certainly could not see the image on the leaves or bark, for even the Kum-Bum lamas, to whom I mentioned my inability to detect anything on the leaves they had given me, assured me that *faith was necessary*—"as one's faith is so is the clearness of the image on the leaf."

I hope the leaves will assist in throwing some light on the question.

Ever sincerely yours,
(Signed) W. W. ROCKHILL.

Sir Augustus Franks kindly sent me some of the leaves, accompanied with the following memorandum:—

"Leaves from the tsandan karpo ('white sandal-wood tree') of Kŭmbŭm, said to have sprung up on the spot where Toongkàpe's mother threw his hair when, having shaved his head, she consecrated him to the house.

"Used when ground as medicine—also carried in charm boxes.

"Collected by W. W. Rockhill at Kŭmbŭm in 1891."

They were carefully examined by Mr. W. B. Hemsley, F.R.S., Principal Assistant in the Kew Herbarium, who has long been engaged on a critical study of the Chinese Flora. He arrived at the conclusion that they belonged to *Syringa villosa*, a Chinese species. He published his determination in *Journ. Linn. Soc.* (vol. xxx. p. 133), and I am disposed to regard it as correct. It confirms the statement of Kreitner (NATURE, xxvii. p. 171).

Rockhill's identification with *Philadelphus* is a mistake easy of explanation. He has confused the popular and the scientific use of the name Syringa. Lilac is botanically *Syringa*; Syringa is botanically *Philadelphus*.

It will be seen from the accounts given above that the phenomenon is not consistent with itself at different times. This confirms the opinion of M. Blanc that it is an elaborate fraud.

W. T. THISELTON-DYER.

P.S.—I have omitted to add that Blanc says (*l.c.* p. 323): "L'arbre paraît appartenir à la famille des Phytolaccacées ou à une famille analogue."—W. T. T. D.

The Röntgen Rays.

REFERRING to two letters in your last issue, p. 388, it is somewhat disconcerting to have Prof. Röntgen's original experiment—viz. the observation of shadows thrown on a barium-platino-cyanide screen—treated as a novelty. No one at all informed can have had the least scepticism concerning the probable observation of such shadows by Prof. Salvioni, though the sensational announcement made by some daily papers, that the eye had been made "actually to see" objects inside enclosures, was received, and is still received, with complete incredulity. A protected barium-platino-cyanide screen is extremely useful as a tester of the condition of an exhausted tube, and I have constantly used it as such, in imitation of Prof. Röntgen.

That a Crookes' tube gets hot when in action is perfectly patent to the touch; so also is the electrostatic attraction of the tube for pieces of paper and the like.

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[1] Kreitner, "Im Fernen Osten," p. 708. I was told that in spring these trees have large clusters of violet flowers, but if they are lilacs I am astonished that the Chinese do not speak of them as such, for that shrub is well known in Kan-su and throughout Northern China (see Prjevalsky. "Mongolia," ii. 79). Tibetans call all sweet-smelling wood *tsandan* (*i.e.* sandal-wood). Sir Joseph Hooker (*Himalayan Journals*, i. 298) says that the Lepshahs and Bhoteas call the funereal cypress *tsandan*. The Kum-Bum tsandan karpo is certainly not a cypress, however.

[2] When Lieutenant Kreitner visited this place (1879), the images on the leaves were as at the present time. See "Im Fernen Osten," p. 707. The Arab traveller, Ibn Batuta, saw in the fourteenth century, at Deh Fattan, on the Malabar coast, in the courtyard of a mosque, a tree called the "tree of testimony." Every year there was a leaf on it, on which was written "by the pen of divine power," the formula, "There is no God but God; and Mohammed is the envoy of God." The inhabitants used it to cure disease (see Ibn Batutah, Defrémery's Transl., iv. 85).

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The term "anode rays" for the rays discovered with so much *éclat* by Prof. Röntgen, whether they be the same as those previously discovered by Dr. Lenard or not, is suggested by remarks from Mr. A. W. Porter at a recent meeting of the Royal Society. They certainly do not start from the kathode, but from some opposed surface, a surface which may be an actual anode, and which always has some anodic properties. From each point of such a surface rays start in all directions; this is proved by the shadows they cast of slits, holes, and wires.

Oliver J. Lodge.

I may state that in a lecture which I gave here on the evening of Tuesday last, the 25th ult., I showed to a large audience, by means of a sheet of barium platino-cyanide, rendered fluorescent by the Röntgen rays from a Crookes' tube, all the things referred to by Mr. Campbell Swinton in his letter in the last number of Nature. The shadows of coins in a purse, and of a hand, were distinctly visible to the audience when placed behind screens perfectly opaque to ordinary light, and, though more dimly, even through a book of eight or nine hundred pages.

I must confess that I cannot see why, after Prof. Röntgen's account of his own work, the success of such experiments as those made by Mr. Swinton or myself should be regarded as surprising, or accounts of them received with incredulity. They seem to me to be merely a variation of Prof. Röntgen's own experiments, or at most to be a matter of the most obvious inference from these experiments.

The statements that have appeared to the effect that Signor Salvioni has devised a method of rendering the retina of the human eye sensitive to Röntgen rays, and that by his method objects are directly *seen* through planks of wood, sheets of aluminium, &c., are simply absurd. The fluorescent light produced is entirely distinct from the Röntgen rays, and affects the retina like ordinary light; and of course parts of the sheet which do not fluoresce, because they are shielded by the opaque objects behind from the Röntgen rays, appear dark. Seeing such shadows can no more be said to be *seeing the objects themselves* by means of the Röntgen rays, than a man can be said to see himself when he looks at his shadow thrown by an ordinary gas-lamp on the street.

Prof. Röntgen discovered the fluorescence of the barium platino-cyanide under the rays now called by his name, and the transparency of ordinarily opaque matter to these rays, and the discoveries of Signor Salvioni and others, so far, at any rate, as they have been described in Nature[1] and other journals I have seen, amount to nothing more. It is only just that in accounts of verifications of Röntgen's discoveries an attempt should be made to show clearly that such observations are only verifications, so as to prevent the credit of discovery which is Prof. Röntgen's due from any appearance, however unintentional, of indirect diminution.

Scientific accounts of verifications, as far as I have seen them in Nature or elsewhere, are in themselves unexceptionable; but extra precaution seems necessary in order that the public should not be led by newspaper paragraphists, retailing such accounts at second-hand, to regard as extensions of Röntgen's work what are only direct and obvious consequences, perceived by himself, of the facts which he has observed.

Andrew Gray.

University College of North Wales, Bangor, March 1.

In your last issue (p. 399), in the account of the work appearing in the *Comptes rendus*, you state that M. de Heen "proves conclusively that the X-rays proceed from the anode and not the kathode." May I point out (as I did at the Royal Society, in the course of the discussion on Prof. J. J. Thomson's paper, February 13), that I have proved that this is undoubtedly true for the bulb that I have been using *throughout* my experiments on the X-radiation. The bulb is one in which the negative electrode is concave, and the negative stream is thereby focussed to a point on the anode, which is a platinum disc placed near the centre of the bulb. By measuring the positions of different parts of a radiograph of a series of concentric zones of tinfoil placed in a measured position, I have shown that the actinic rays diverge from the anode disc.

I am of opinion, however, that in this respect this bulb differs from those which have been employed by others. In these latter, judging from the published accounts, the negative stream impinges directly on the glass; and for bulbs of this kind, it has been shown conclusively by Prof. J. J. Thomson that the seat of the origin of the rays is the glass itself. The proof is that a sensitive plate placed inside the bulb in the path of the negative stream is not acted upon. I venture to think that, in the case of my bulb, a sensitive plate placed inside would be acted upon provided it lay in the hemisphere of the bulb in which the kathode lies. I intend to test this conjecture experimentally. Should it prove true, the behaviour of both varieties of bulb will probably be capable of description by the following single statement:—

The seat of the origin of the X-rays is where the negative stream first impinges against a solid, and gives up, or partially gives up, its negative charge.

Alfred W. Porter.

University College, London, February 28.

In your "Notes" of last week you refer to a communication of M. de Heen, stating that the X-rays proceed from the anode. Some experiments, which I made at the beginning of last month, bearing on this point, may be of interest to your readers. It is of course not the case that the X-rays proceed from the anode in general, but they may be made to do so by placing a small disc as anode facing the kathode. The kathode streams impinge on the former, and the X-rays being generated there radiate from it. The experiment was made by placing a lead plate (4 cm. by 10 cm.), with a rough circular hole in it, at a distance of 10 cm. above the photographic plate, and the tube (a small one with a curved kathode facing a small disc anode) 10·5 cm. above the lead. After development the negative was replaced exactly in its former position. Several interesting facts showed themselves, the most striking being that the image of the hole consisted of a well-defined circle showing even individual splinters on the edge, and in addition diffused elongation on two sides. On placing the eye so that the hole exactly covered its well-defined image, it was necessary to put it in the position occupied by the anode.

The diffused images in the same way were seen to be due to the fluorescent parts of the glass sides of the tube—a kind of pin-hole photograph, in fact.

The rays leave the anode as if they were the splash of a jet of water occupying the position of the kathode stream. In other words—supposing the plane of the anode vertical and the kathode to the right, then no rays appear to the left of the plane of the anode, whilst on the right the space is exceedingly rich. The negatives show, in every case tried, two well-defined regions, viz. nothing to the left of the intersection of the plane of the anode with the negative, and a dense deposit on the right, the richest part apparently being close to this line. It is difficult to account for this on the supposition that the rays are due to waves generated at the point of impact. We should expect in this case the action on the plate to increase with the visual angle of the anode disc as seen from points on the plate. On the other hand, certain further experiments seem to show that the action is not in all respects similar to the splash of a jet. Whether the effect is due to the fact that the place of impact of the kathode stream is an anode, or simply an internal obstacle, I have not yet determined, but experiments in progress will, I hope, settle this point.

For photographic purposes, the best kinds of tube are those with a curved kathode converging the streams on a small plane anode, remembering, of course, that the strong field is on the kathode side of the anode plane. This behaves very approximately as a radiating point. With this I have obtained, with comparatively short exposures, and a 2½″ to 3″ spark length, strong negatives of remarkable definition, certainly finer than any I have yet seen.

W. M. Hicks.

Firth College, March 1.

I was interested to see in your last issue a letter from Mr. Swinton describing his reproduction of Prof. Salvioni's experiments with phosphorescent screens. Mr. Swinton uses a piece of blotting-paper impregnated with platino-cyanide of barium. I have tried this method, but have obtained better results with a screen prepared with the same salt, as follows:

A piece of fairly stout black paper, free from pinholes, is coated with gum containing a little glycerine, and, as soon as it has

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[1] A translation of Prof. Salvioni's paper will be found in another part of this issue.—Ed. Nature.

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